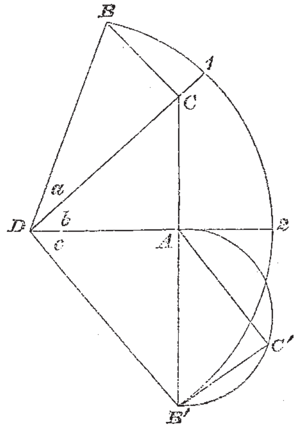
arc B1, which is in the same plane with it, will be at right angles to the arc 12. The third arc 2B′ will therefore be the hypothenuse of a right-angled spherical triangle, of which B1, 12, are the two sides. Calling these arcs or the angles of the faces resented by them, *a*,*b*,*c*, and the angles opposite to them in the spherical triangle, A,B,C, the proof of Napier's Rules, with this solid figure, proceeds by the same direct steps as those already described, with a special example of the figure in my former letter. As the construction there described is confined to the representation of a particular kind of right-angled spherical triangle, and is therefore inapplicable to illustrate the proof of Napier's Rules experimentally in every given case, the general construction supplied by "J. J. W.," which is limited by no such restrictions, and which is at least equally convenient, will evidently serve more effectively the same practically useful and instructive purpose.



Instead of "accessible," as applied to the difficulties of the geometrical proofs produced by Mr. Cooley in his letter on "Elementary Geometry" (in NATURE, No. 103), which are indeed there obviously overcome, I would have used the word "surmountable" as more descriptive of geometrical difficulties, properly treated and discussed, had the word immediately presented itself to me; but having often found an easily executed model extremely useful and convenient in practical applications of Napier's Rules, with whose design, as a general resource to facilitate their study, I was not, however, so fully satisfied, I applied, perhaps unconsciously, to Mr. Cooley's demonstrations a term expressing strictly only the diffidence with which I ventured to present to readers of NATURE my own very imperfect geometrical contrivance. In thus making my difficulties accessible to "J.J.W.," I very gratefully acknowledge the assistance which I have derived from his remarks on my letter in NATURE, No. iii., and I cheerfully admit the merit and superiority of the general rule for constructing a proper model in cardboard, to illustrate the proofs of Napier's Rules, and to facilitate their study, which he has kindly consented to describe.

Newcastle-on-Tyne, Dec. 16 A. S. HERSCHEL

Alternation of Generations in Fungi

I AM sure that the Rev. M. J. Berkeley will exonerate me from any deliberate intention to misrepresent him; nor do I think that there is, after all, much difference of opinion between us regarding the present subject, unless, perhaps, that I am more sceptical. I alluded to the paper cited by him from the "Journal of the Horticultural Society," on propagation of bunt spores, and not to his communications on the hop or vine mildew. I was under the impression that he regarded the "four consecutive forms of reproductive cells in the bunt" as an instance of alternation of generations. On reference to the original paper, I find that he did not go so far then as to indicate four consecutive forms of reproductive cells; but that Tulasne followed on his track in 1854, and in 1857 Mr. Berkeley seemed to have accepted the results of Tulasne's observations, since, in his "Introduction," he gives figures at page 318, in the description of which the following phrases occur:—"spores of the second order," "spores of the third order," "spores of the fourth order." Here are the "four consecutive forms of reproductive cells" to which I alluded. At page 321 he writes concerning the bunt:—"The spores, however, are not immediate means of propagation; they are, in fact, only a sort of prothallus, from which the mycelium grows, producing at the tips, or on lateral branchlets, bodies of various forms, which are themselves capable of germination, and immediately reproduce the species." The real issue between us seems to lie in the phrase, "alternation of generations." If the bunt spores, on germination, produce fusiform bodies, which, after conjugation, produce short cylindrical spores, and thus intermediate reproductive cells unlike the parent cell come between that and the ultimate reproduction of the species, I am induced to call it an "alternation of generations." It would be waste of time to discuss phrases, or I might take exception to the application of this phrase to the *Erysiphei*. The conidia and pycnidia of the hop mildew may be developed without sporangial conceptacles, and the parasite reproduced without sporangial fruit, but I cannot recognise alternation of generations in the reproduction of a species by means of conidia, stylospores, or sporidia, or by one of these alone. If such may be construed into an alternation of generations, it must be by permitting greater elasticity to the phrase. Conidia germinating and producing pycnidia, the stylospores of the pycnidia germinating and producing sporangial conceptacles, containing the sporidia which, upon germination, will produce the mycelium and conidia again, returning to the original form after two or three consecutive departures from it, appears to me a perfect type of alternation of generations. I fully admit that "if it is once established that a Puccinia produces an Æcidium, or an Æcidium a Puccinia, we should have a clear case, especially when the third form reverts to the first again." Without the slightest desire to "depreciate the labours of Oersted and De Bary," I cannot admit that they have established facts until their observations are confirmed, especially when there is an evident possibility of their having been deceived. I shall have no hesitation in accepting the facts when they are confirmed by independent and equally trustworthy observers, although I may be unable to account for some of the phenomena. At present I must confess that I am not so sanguine as Mr. Berkeley appears to be.

The correspondent signing himself "Mycelium" wishes to know if "the liability to produce parasitic fungi is communicated from the seed to the mature plant." In some instances we know such to be the case, in others perhaps only suspect it. The "bunt" is an instance, or why the steeping of seed corn? or how did the Rev. M. J. Berkeley succeed in producing bunted wheat plants from seed corn inoculated with bunt spores? Two or three years since I published particulars of a similar instance of celery seed and *Puccinia Apii*. It would be as rash to affirm that this is always the case as to deny its occurring at all.

M. C. COOKE

In Re Fungi

THE letters in your last two numbers have reminded me how ill this subject is studied by some botanists in this country. I will give two recent instances: 1. In the last number of the *Journal of Botany*, p. 383, it is positively stated that *Agaricus cartilagineus* (a rare and very critical species by the way) was determined by a growth which is there described a mere *mass of mycelium*. He must have been a bold man who ventured to name an agaric (above all things) from a *mass of mycelium*. 2. In the first number, October 1871, of the *new edition* of "Paxton's Botanical Dictionary"—"enlarged and revised"—under the article *Agaricus* there is to be found such a collection of obsolete names and absurd errors as to make the article simply ridiculous.

W. G. S.

Mr. Lowne and Darwinian Difficulties

MR. LOWNE (NATURE, December 7) sees no difficulty whatever in explaining by what natural process an insect with a suctorial mouth is developed from one having the mandibular type of mouth, but still he does not explain. He affirms there is no doubt that "the pupa state is a modification (!) of the ordinary process of skin shedding," and that this is "proved" by so many facts that he cannot understand how it could be "denied," &c., but he does not prove it.

For aught I can tell, every internal tissue and every external scale of the butterfly may be represented in the larva; but I do not know and cannot prove that this is so, nor do I believe any one can prove it. That the changes which take place during the pupa state are very different from those that occur during any portion of the larva period, will be admitted by every one who

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has kept silkworms or bred butterflies. The assertion that there is absolutely only a difference in the time at which the successive skins are formed in this and in ordinary ecdysis, is but assertion on the part of Mr. Lowne. Indeed, controversy becomes profitless if authority is to be substituted for fact, and an attempt made to silence opponents and stop inquiry by such positive assertions as the above and the following :—"The imaginal skin is likewise derived from cells laid down in contact with the imaginal discs." If Mr. Lowne will be so good as to explain what no books tell me, and I fail to make out myself, I will study what he says with great attention, and thank him heartily. He knows me well enough to feel assured that I would do so ; but it is useless, and he must permit me to say that it is not in good taste, for him to comment about the "return of darkness," and to use expressions more positive and arbitrary than are called for.

Let us, if we can, get at the facts concerning some of these marvellous changes. For this there is nothing like discussion, carried on with care and consideration, even for an opponent ; and though the fittest may be certain that he will survive, don't let any one be in too great haste to proclaim himself either survivor or fittest, or call himself strong and others weak, as has been done once already by one distinguished evolutionist. Evolution is a much quieter and far more complex process than some enthusiasts would have us believe.

Mr. Lowne appeals to the fly. By all means let the fly be the subject of our inquiries. Of this creature he says, the nervous system undergoes *modification* but not *degeneration*. Now I ask, what part of the nervous system that is present in the maggot can Mr. Lowne find in the fly ? I have studied both fly and maggot carefully, have worked at the matter long, and have utterly failed to find a trace of the nerve tissue of the maggot in the fly. Not only so, but I find the nerves of the fly as different as are the muscles from those of the maggot. The latter are altogether distinct in structure and in action. They contract at a very different rate, and are very different in many particulars.

Again, I must ask Mr. Lowne if he has seen any vestige of the mouth organs in the larva, for he says :—"It is the mouth organs of the larva which are new formations, not those of the imago." I have failed in my attempts to find any traces. There are other assertions about the alimentary canal and the sexual organs which are not proved. Does Mr. Lowne mean to say, for instance, that he or anyone else can adduce any reliable observations to prove that "the sexual organs are gradually developed, even from the time when the embryo is enclosed in the egg" ? On p. 112 of his book on this very matter he says that he has not been able to verify Dr. Weissmann's assertion as to their presence, even in the larva ; and now he suggests they exist in the egg !

But I must ask Mr. Lowne to explain what he means by saying in his letter, that it is an "utter mistake to suppose that any insect is re-developed during the pupa state," and that the nervous system "never undergoes degeneration ;" because on p. 116 of his own book, published only last year, I find the following passage : "All the tissues of the larva *undergo degeneration*, and the imaginal tissues are *re-developed* . . . under conditions similar to those appertaining to the formation of the embryonic tissues from the yolk" !

LIONEL S. BEALE

The Auditory Nerves of Gasteropoda

IN your issue for October 26, I notice an account of Leydig's recent paper on the auditory organ of the Gasteropoda, which, though excellent in other respects, has an error of omission which I should like to see rectified. When so important a discovery for morphology is discussed as that of the innervation of the otolithic sac from the supra-œsophageal in place of the sub-œsophageal ganglion which is its apparent connection in all Gasteropoda (excepting the Heteropodous forms), the credit of it should be given to the right man. That man is the most eminent and accurate of French comparative anatomists—M. Lacaze-Duthiers. Prof. Leydig states in the beginning of his own paper that Lacaze-Duthiers' statements on this subject (published in the *Comptes Rendus* about three years ago, if my memory serves me, and curiously mistranslated, *sus-œsophagien* being rendered *sub*-œsophageal in one of the first numbers of the Monthly Microscopical Journal), caused him to direct his attention again to this subject, and he has, as a result, confirmed the observations of the French *savant*, which were in opposition to the previously-received views of all observers, himself and Leydig included. Germany has a host of indefatigable anatomists, and the services of Franz Leydig, of Tubingen, are brilliant enough to eclipse most zootomical reputations; but let us not, at this moment above all others, forget to do justice, when the opportunity occurs, to a naturalist whose comprehensive, accurate, and beautiful zootomical monographs, rich in discoveries, have done more than those of any other Frenchman to sustain the great name of Cuvier's school.

Naples, Dec. 8 E. R. LANKESTER

DR. CARPENTER AND DR. MAYER

AT the Anniversary Dinner of the Royal Society on November 30, I was honoured by a request from the President to say a few words in acknowledgment of the toast to the Copley Medalist. I did so, stating briefly the origin of my acquaintance with Dr. Mayer's writings. Though Dr. Carpenter at the time was within sight of me, it did not occur to me to introduce his name into my remarks. A few days afterwards I was favoured by a letter from Dr. Carpenter, in which he reminds me somewhat sharply of this and other lapses as regards himself, and requests me to rectify the omission by a brief communication to the *Athenæum* or to NATURE. It will be fairer to Dr. Carpenter, and more agreeable to me, if he would state his own case *in extenso*. Here is his letter :—

"University of London, Burlington Gardens, W.,
"December 5th, 1871.

"MY DEAR TYNDALL,—If I correctly apprehended what you said at the Dinner of the Royal Society in regard to Dr. Mayer, you repeated what you had previously stated in your Lecture at the Royal Institution in 1863, as to the entire ignorance of Mayer's work which prevailed in this country until you brought it into notice on that occasion.

"Now, I very distinctly remember that a few days previously to that Lecture, I mentioned to you that as far back as 1851 I had become acquainted, through the late Dr. Baly, with one of Dr. Mayer's earlier publications ; and that, in bringing before the readers of the *British and Foreign Medical Review* (of which I was then the Editor) the 'Correlation' doctrine, as developed in Physics by Grove, and in Physiology by myself, I had stated that we had both been to a great extent anticipated by Mayer—as I should have shown much more fully if the pamphlet had earlier come into my hands.

"I also most distinctly remember that, as you stated in that Lecture, no one in this country—'not even Sir Henry Holland, who knows everything'—had ever heard of Mayer, I spoke to you again on the subject a few days afterwards ; and that you then expressed your regret at having entirely forgotten what had previously passed between us on the subject.

"As it would seem that this second mention of the matter has also passed from your mind, I shall be obliged by your looking at the passages I have marked in pp. 227 and 237 of the accompanying volume, from which I think that you will be satisfied that I had at that date correctly apprehended Mayer's fundamental idea, and that I have done the best to put it before the public that I could under the circumstances—the article having been in type and ready for press before his pamphlet came into my hands.

"Since, in thus bringing forward Mayer, I spontaneously abdicated the position to which I had previously believed myself entitled, of having been the first to put forward the idea that all the manifestations of Force exhibited by a living organism have their source *ab extra*, and not —as taught by physiologists up to that time—*ab intra*, I venture to hope that you will do me the justice of stating the real facts of the case in a short communication either to the *Athenæum* or to NATURE.—I remain, my dear Tyndall, yours faithfully, "WILLIAM B. CARPENTER

"Prof. Tyndall."

This letter was accompanied by a volume of the *Medico-Chirurgical Review*, containing an article headed, "Grove, Carpenter, &c., on the Correlation of Forces,

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